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Climate of the Past Discussions

Interactive comment on "Heinrich Event 4 characterized by terrestrial proxies in southwestern Europe" *by* J. M. López-García et al.

Anonymous Referee #1

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I think that this is an interesting work on the application of one of the classical methods for the development of palaeoecological analyses based on vertebrate communities: the mutual climatic range method. It also includes an analysis on the chorological characterization of mammalian species, which in my opinion is a very promising line of research.

Nevertheless, although the topic is very interesting there are some points that must be kept in mind.

3.1. Small-vertebrate sorting and palaeontological study

The authors do not make any assessment on the completeness of the assemblage. Are 182 identifiable specimens and a MNI=26 enough for the adequate implementa-

tion of the MCR method? How many species may lack due to low number of fossils? The authors should discuss how MCR behaves when facing poorly sampled assemblages. I am not saying that the fossil site has been poorly sampled, but sometimes the assemblages recovered are not adequate for palaeoecological studies because the may not be rich enough to adequate reflect the original palaeocommunity. The authors should argue on this issue on page 651.

3.2. Palaeoenvironmental reconstruction

I do not see the difference between "mid-European species tolerant of Mediterranean conditions" (chorotype 2) and "non-strictly Mediterranean species" (part of chorotype 3). Please, explain clearly the differences between the species included in these two chorotypes.

3.3. Palaeoclimatic reconstruction

What are the potential consequences of mixing such different taxa as mammals, reptiles and amphibians when using the MCR method? Is there any work that has empirically shown that there is no problem with that? The reference cited (Blain, 2009) only indicates the use of herps.

In any case, my main concern with the methodology is the use of Iberian distributions of the taxa recorded for the development of climatic envelopes for them. The authors should use global distributions of the taxa because the restriction of the climatic characterization of each species to its Iberian range surely is constraining the climatic information that is associated to the whole species. For example, the range of Microtus arvalis includes steppe areas in Central Asia with climatic values very different to anything found in the Iberian Peninsula. Similarly, Microtus agrestis may be found in tundra ecosystems with no climatic analogue in the Iberian Peninsula. This affects to the climatic inferences obtained by the MCR. This contrasts with the interest of the authors for the understanding of the potential ecological/climatic distribution (page 653, lines 4-8).

Therefore, the authors should discuss extensively why they use this restrictive version of the MCR, unless I am wrong and they effectively are using global distributions (in this case they must state it clearly in the paper).

5.3. Discussion

In general, I found very difficult to follow the arguments in this paragraph. This is partially due to the scattered information in Fig. 8. In order to understand whether the climatic information provided during the different Heinrich events is significant, we should know the climatic values between them, which is not shown. I understand the difficulties associated to the scarce fossil record at this resolution level. Therefore, I would suggest to the authors an additional effort in the argumentation associated to this section.

Minor points

* Page 651 (line 10): "Bailon (1999), and Sanchiz et al. (2002), Blain (2005, 2009)..." should be corrected to "Bailon (1999), Sanchiz et al. (2002), Blain (2005, 2009)...".

* Page 651 (line 13): "vertebrae for lizards and vertebrae for snakes..." may be corrected to "vertebrae for lizards; vertebrae for snakes...".

* Page 651 (lines 21-22): If I am not wrog the correct name for Evans et al's method is Taxonomic Habitat Index method. Please, check it.

* Page 651 (lines 26-27): Please, remove the habitat list. Each one of them is extensively explained just following the next line. Thus, there is no need to be redundant.

* Page 652 (line 16): Although I share the view that the Iberian Peninsula has many singularities within the European context, I would not use the word "minicontinent" to define it.

* Page 654 (lines 11-12): I do not understand this sentence: what is the meaning of (MNI = 39) within the context of the sentence? Additionally, please, clarify the connec-

tions with the previous sentences.

* Page 654 (line 13): Which hypothesis are the authors referring?

* Page 655 (line 28): "are very well represented at Canyars, often accounting for more than 40% of the total sample". This sentence appears to indicate the existence of different levels al Canyars, which have not been described before in the manuscript. Is this true? Or perhaps the word "often" should be removed from the sentence.

* Pages 658-659: When mentioning percentages of chorotypes (60%-40%), the numbers do not agree with figure 6 (50%-50%). Please check which one is accurate and correct the manuscript accordingly (not only numbers, but also associated sentences).

* Page 659 (line 1): Artemisia should be in italics, right?

* Page 659 (lines 6-29): These three paragraphs are showing more or less the same information than in the previous section. I would suggest to remove them and, perhaps, include the new pieces of information within the previous paragraphs.

* Page 660 (line 16): Where it is mentioned the Fig. 7, I think it should refer to Fig. 6.

* Page 661 (line 14): "MAT" should be changed to "MAP".

* Page 662 (line 17): "This assemblage is dominated by mid-European taxa". Are the authors referring to species richness or to abundance? According to Fig. 6 Mediterranean taxa are half of the association (see comment above).

* Table 3: I would suggest to include a new column indicating the deviation from modern values of these climatic parameters. Although most of the modern values (Barcelona airport) are indicated in page 653 (lines 16-20), I think that inclusion of this information in the table would easy the comparison for the reader.

* Fig. 5: The percentages associated to each skeletal group do not sum 100%. Derived from the last sentence in the figure caption, I guess that this is due to the presence of unclassified remains. I would suggest the inclusion of a new histogram for these re-

mains unclassified according to the degree of digestion. This would make clearer the figure. Additionally, the inclusion of numbers of specimens (more than only percentages) for each column would serve as an indicator of the sampling associated to each digestion degree.

I hope that the authors of this attractive paper consider these suggestions interesting and adequate.

Interactive comment on Clim. Past Discuss., 9, 647, 2013.